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ARE THERE CIVIC RETURNS TO EDUCATION?

Thomas S. Dee

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The hypothesized effects of educational attainment on adult civic engagement and attitudes provide some of the most important justifications for government intervention in the market for education. In this study, I present evidence on whether these externalities exist. I assess and implement two strategies for identifying the effects of educational attainment. One is based on the availability of junior and community colleges; the other, on changes in teen exposure to child labor laws. The results suggest that educational attainment has large and statistically significant effects on subsequent voter participation and support for free speech. I also find that additional schooling appears to increase the quality of civic knowledge as measured by the frequency of newspaper readership.

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"...since the achievement of American Independence, the universal and ever-repeated argument in favor of Free Schools has been, that the general intelligence which they are capable of diffusing, and which can be imparted by no other human instrumentality, is indispensable to a republican form of government."

Horace Mann (1846)

INTRODUCTION

Economists typically justify the government's extensive and varied involvement in the market for education by appealing to distributional concerns and several types of market failures. The most frequently discussed types of market failure involve the positive externalities that might be associated with schooling. For example, some have argued that education generates external social benefits by reducing the prevalence of crime and by promoting knowledge spillovers and technology diffusion in the workplace.[1] However, the externality that is arguably featured most prominently in discussions about education involves civic behaviors and attitudes. Specifically, it is widely believed that education is an essential component of a stable democratic society because it encourages citizens to participate in democratic processes and prepares them to do so in an informed and intelligent manner. The putative existence of such civic returns to education motivated the proliferation of common schools in the early 19th century and early educational reformers like Horace Mann and continues to provide one of the most important justifications for the many public policies and institutions that promote access to all levels of education.

An extensive, empirical literature in political science has documented a strong correlation between educational attainment and various civic behaviors. In particular, this literature has demonstrated that higher levels of schooling are associated with substantive increases in voter turnout. Political scientists generally interpret this literature as providing strong support for the view

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[1] See Wolfe and Haveman (2001) for a discussion of the non-market and social benefits possibly associated with education. Poterba (1996) and Taylor (1999) discuss the case for governmental intervention in the market for education and conclude that there is surprisingly little empirical evidence to indicate whether or not hypothesized, positive externalities exist. However, several recent empirical studies have assessed the effects of schooling on knowledge spillovers (e.g., Moretti, forthcoming, Acemoglu and Angrist 2000) and on criminal behavior (Moretti and Lochner 2001, Witte 1997).

that education is effective at promoting the quantity and quality of civic participation. However, these correlations could actually be quite misleading since both schooling and civic outcomes are simultaneously influenced by a wide variety of inherently unobservable traits specific to individuals and the families and communities in which they were reared. For example, individuals who grew up in cohesive families and communities that stressed civic responsibility may also be more likely to remain in school. The plausible existence of such unobservables implies that conventionally estimated correlations may spuriously overstate the true civic returns to education.[2]

This study attempts to construct less ambiguous empirical evidence on this policy-relevant issue by identifying the causal effects of additional schooling on civic behaviors and knowledge. The research designs adopted here essentially parallel the extensive, empirical literature on the labor-market returns to schooling (e.g., Angrist and Krueger 1999, Card 1999). More specifically, these inferences rely critically on instrumental variables that generated possibly exogenous variation in individual levels of schooling but that should otherwise be unrelated to adult civic outcomes.[3] First, using data from the High School and Beyond (HS&B) longitudinal study, I estimate the effects of college entrance on adult voter and volunteer participation by relying on the geographic proximity and density of junior and community colleges as a teen. Then, using data from the 1972-2000 General Social Surveys (GSS), I estimate the effects of years of schooling on adult voter participation, on group memberships and on attitudes towards free speech by relying on changes in teen exposure to child labor laws (Acemoglu and Angrist 2000). Using the GSS data, I also estimate the effects of additional schooling on the frequency of newspaper readership, an outcome that is closely related to measures of civic awareness. The results of these evaluations suggest that additional schooling, both at the secondary and post-secondary levels, had large and statistically

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[2] An additional concern is that the existence of measurement error in self-reported schooling could lead correlations to *understate* the true effects of schooling (Angrist and Krueger 1999, Card 1999). The direction of omitted variable biases could also be negative. For example, the high-ability individuals who continue their schooling may have higher opportunity costs and may think that voting is largely an expressive act that is extremely unlikely to actually influence policy.

[3] I discuss a variety of ad-hoc empirical evidence that is consistent with the maintained assumptions regarding instrument validity.

significant effects on voter participation. I also find that the additional secondary schooling significantly increased the frequency of newspaper readership as well as the amount of support for allowing most forms of possibly controversial free speech.

EDUCATION AND CIVIC ENGAGEMENT

One of the fundamental mechanisms by which education has long been thought to generate civic externalities involves improvements in the quality of civic participation and awareness. Specifically, it is widely alleged that increases in education generate broad social benefits by allowing citizens to make more informed evaluations of the complex, social, political and technological issues that might be embedded in campaign literature, legislative initiatives and ballot referenda. However, the contemporary literature among political scientists has also put a particular stress on the positive effects that schooling may have on the likelihood of civic participation (e.g., Wolfinger and Rosenstone 1980). Education could promote civic participation through at least two broad channels. First, schooling may reduce the effective costs of certain forms of civic participation. In particular, this is thought to occur because increased cognitive ability makes it easier to process complex political information, to make decisions and to circumvent the various bureaucratic and technological impediments to civic participation.[4] Second, education may increase the perceived benefits of civic engagement by promoting "democratic enlightenment" or, stated differently, by shaping individual preferences for civic activity. Similarly, it is often alleged that education plays an important public role by directly inculcating students with other fundamental democratic and pluralistic values (e.g., support for free speech, for the separation of church and state, etc.).[5]

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[4] However, there are other indirect mechanisms by which education may currently lower the effective costs of voting. For example, since thirteen states currently prohibit ex-felons from voting, education may also reduce the effective costs of voting through its effects on criminal activity. Similarly, to the extent that education increases the likelihood of having a driver's license, the recent expansion of "motor-voter" policies may have added to the effects of education on voter turnout.

[5] The preference-shaping nature of schooling is typically viewed as normatively desirable. However, Lott (1990, 1999) argues that governments use the indoctrination that occurs in pubic schools to support totalitarian regimes and large wealth transfers.

However, it is also possible that additional schooling shapes civic preferences indirectly through altering the composition of peer groups and shared social norms.

Interestingly, an economic perspective could also suggest alternative mechanisms by which additional schooling might actually reduce civic engagement. For example, by raising the opportunity cost of an individual's time, increased schooling could reduce the amount of time and attention allocated to civic activity. This could be particularly relevant for volunteering, which, unlike voting, can involve a substantial commitment of time. However, education could also reduce voter participation by promoting an awareness of voting as an essentially expressive act with an infinitesimally small probability of influencing actual policy.[6] Nonetheless, the available empirical evidence seems to provide an emphatic confirmation of the conventional view that education does promote civic engagement. Numerous studies over the last fifty years have demonstrated that higher levels of individual schooling are strongly associated with civic behaviors and knowledge.[7] For example, in a widely repeated interpretation of this empirical evidence, Converse (1972) refers to educational attainment as the "universal solvent" of political participation. Similarly, Putnam (2001) notes that "education is by far the strongest *correlate* that I have discovered of civic engagement in all its forms" (emphasis mine). Also, in their earlier study of voting participation, Wolfinger and Rosenstone (1980) suggest that their core finding is the "transcendent importance of education." However, they also note that an individual's level of schooling could easily proxy for unobserved traits that also influence civic behaviors (pages 19-20). For example, they suggest that the types of family backgrounds that promote increased schooling may also promote increased socialization into civic activities like voting. Wolfinger and Rosenstone (1980), like other researchers in this field, have attempted to control for the possible bias in the estimated effect of education by introducing a few additional control variables (e.g., income and occupational measures) into multiple regression

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[6] See Mueller (1989) for a discussion of the paradox of voting, models of voting behavior and issues related to the quality of the vote.

[7] See Nie, Junn and Stehlik-Barry (1996, page 3) for extensive references to this empirical literature.

models. The apparent robustness of the correlations between education and civic outcomes has led most researchers to conclude that education does have a causal effect. For example, in the most recent contribution to this literature, Nie and Hillygus (2001) note that this orthodox view is "largely uncontested."

However, the basic approach of introducing a few additional controls may not convincingly resolve the question of whether the strong correlations between education and civic outcomes actually reflect the true causal effects. In particular, this could occur because so many of the shared determinants of civic behavior and educational attainment are inherently difficult for researchers to measure well. For example, as noted earlier, children who were raised in families or communities that stressed civic responsibility are almost certainly more likely to remain in school longer. This may occur in part because such families and communities are also likely to impart values that encourage schooling. However, it could also occur simply because civic-minded families and communities may do more to insure that their children attend well-funded, high-quality schools. These plausible scenarios imply that the strong association between adult civic outcomes and educational attainment may reflect, to an unknown degree, the confounding influence of unobserved family and community traits. Alternatively, these correlations could also reflect the confounding influence of other, inherently unobservable individual traits like the rate at which future outcomes are valued and the taste for altruism. Certainly, the recent trends in the United States (i.e., declines in voter turnout and contemporaneous increases in educational attainment) suggest that the association between education and civic engagement could be specious. And at least two studies in the political science literature provide more formal evidence that such concerns about omitted variable biases may be empirically relevant. Both Luskin (1990) and Cassel and Lo (1997) present evidence that the apparent influence of education on civic outcomes (political literacy and sophistication) may reflect the spurious influence of other individual traits (e.g., intelligence and parents' socio-economic status). Similarly, Gibson (2001) presents within-twin estimates, which suggest that education actually reduces the probability of volunteering. In the next two sections, I present new empirical

evidence on the effects of educational attainment on several civic outcomes. I attempt to identify the causal effects of educational attainment by relying on instrumental variables that generate plausibly exogenous changes in the levels of individual schooling.[8]

COLLEGE ATTENDANCE AND CIVIC PARTICIPATION

High School and Beyond (HS&B)

The data for this section are drawn from High School and Beyond (HS&B), a major longitudinal study conducted by the U.S. Department of Education. This detailed study began with a cohort of high school sophomores in 1980. Follow-up interviews of roughly 12,000 members of the sophomore cohort occurred in 1984 when most respondents were 20 years old and again in 1992 when most respondents were 28 years old.[9] In the 1992 interview, respondents were asked four civic-related questions: whether they were currently registered to vote (mean=.67), whether they had voted in a local, state or national election within the past year (mean=.36), whether they had voted in the 1988 Presidential election (mean=.55) and whether they had volunteered in the last month (mean=.37). The key measure of educational attainment examined here is college entrance defined as of the 1984 interview (mean=.54). This definition of college entrance is based on attendance at a junior college, a community college or a four-year college or university and explicitly excludes those who only attended a vocational, trade, business or other training school. While this is a somewhat narrow margin of educational attainment, the available evidence indicates that it is also an increasingly important one. The rate of college enrollment among young adults has increased dramatically over the last twenty years with roughly half of this increase being absorbed by junior and community colleges (Kane and Rouse 1999). And prior studies suggest that modest persistence at two and four-year colleges has beneficial labor-market consequences even when it does not result in a degree (e.g., Kane and Rouse 1995). The HS&B respondents who had entered college by 1984

[8] A recent study by Milligan et al. (2003) applies a similar methodology to different data sets and finds results similar to those presented here.

[9] See the data appendix for further information on the study and the extract used here.

did generally remain in college long enough to accumulate a relatively large amount of undergraduate credits.[10] Furthermore, the baseline evidence discussed below demonstrates that this measure of college entrance has a strong partial correlation with the probability of subsequent civic engagement. However, the choice of college entrance as a measure of educational attainment is also dictated by the availability of a plausible instrument, the geographic availability of junior and community colleges as a teen, which appears to have substantively influenced the decision to attend college and to have been otherwise unrelated to civic engagement as an adult.[11]

Baseline estimates

The validity of the geographic availability of junior and community colleges (hereafter referred to as two-year colleges) as a basis for identification is a critical issue, which is discussed in some detail below. However, before turning to an assessment of the relevant instrumental variables, it is useful to establish an empirical baseline by estimating the effects of college entrance on subsequent civic behaviors in specifications that assume the absence of omitted variable biases. Table 1 presents the estimated marginal effects from single-equation probits in which the four measures of civic behavior are the dependent variables. The first specification (column (1)) conditions on 10 variables representing basic demographic information on age, race, ethnicity, gender and religious affiliation and on 18 other variables that reflect family income, family composition and parental education as defined during the 1980 interview.[12] The subsequent models introduce school-level controls (i.e., miles to the nearest 4-year college and urbanicity fixed effects),

[10] Specifically, data from the HS&B transcript study indicate that those who had entered college by 1984 had, on average, about 80 more semester hours of undergraduate credit than those who did not: the equivalent of roughly five full-time college semesters. However, a caveat is appropriate since transcript data are missing or incomplete for roughly 25 percent of the respondents in this extract.
[11] Similarly, I chose to define college entrance as of the 1984 interview when most respondents were 20 years old and not as of later interviews. The estimated effects of college entrance are similar regardless of which interview is used to define it. However, college entrance defined as of later interviews had a plausibly weaker relationship to one of the early measures of college availability (i.e., the distance in miles from the high school to the nearest two-year college).
[12] See the appendix for details on these controls. The Huber-White standard errors are adjusted for clustering at the school level.

state and county-level controls based on the location of the base-year school and, finally, fixed effects for the Census division of the base-year school. One of the county-level variables is a well-measured proxy for the civic attitudes of the community in which the respondents grew up: the county-level voter turnout in the 1980 Presidential election. The second county-level variable is a measure of adult educational attainment in the respondent's teen community: the percent of adults aged 25 or older with high school degree. The third county-level control, the population share aged 18-24, may be a relevant determinant of civic engagement and also influence the competitiveness of post-secondary institutions. The two state-level variables reflect influential voter regulations defined as of 1992 (Knack 1995). One is a binary indicator for whether the state had an active policy of allowing voter registration by mail. The second is the number of years the state had active "motor-voter" regulations in place.[13] The available evidence suggests that a years-based measure is the appropriate variable for identifying the early effects of "motor-voter" policies because state drivers licenses are renewed in cycles as long as six years (Knack 1995).

These models are somewhat unusual in comparison to the prior literature since they condition on detailed individual and community-level socioeconomic variables defined as of each respondent's teen years. The results presented in Table 1 uniformly suggest that college entrance had positive and statistically significant effects on civic participation. Interestingly, the magnitudes of these estimated marginal effects are also quite robust to the introduction of the additional controls. These estimated marginal effects are also quite large, implying that a relatively modest increase in educational attainment has sizable effects on subsequent civic participation. For example, these estimates imply that college entrance increased voter registration by approximately 13 percentage points, an increase of nearly 20 percent in the mean probability of being registered. Similarly, these estimates imply that college entrance increases the mean probability of voting in the last year, voting

[13] "Motor-voter" regulations bundle an application for voter registration with those for driver licenses. All states were required to institute "motor-voter" policies by 1995 as part of the National Voter Registration Act. It should also be noted that North Dakota does not have voter registration. The results reported here are robust to excluding observations from respondents who attended high school in that state.

in the 1988 Presidential election and volunteering by 29 percent, 32 percent and 20 percent, respectively.

However, the central concern with the results in Table 1 is that the strong partial correlations between college entrance and civic behaviors may reflect the confounding influence of unobserved determinants of both schooling and civic engagement. One straightforward way to assess the possible empirical relevance of this concern is to examine the partial correlations between college entrance and measures of civic attitudes and knowledge that preceded attendance in college. I rely on two such measures based on data from the sophomore-year survey. One is a standardized test score on questions related to civics. The other is the student's response to a question about the importance of correcting social and economic inequality (1=not important, 2=somewhat important and 3=very important). Each of these variables is highly predictive of each measure of future civic engagement. For example, a 10 percent increase in the sophomore-year civics test score is associated with a statistically significant 7 percent increase in the mean probability of voting within the last year. Similarly, a one-unit increase in the ordered attitudinal measure is associated with a statistically significant 10 percent increase in the mean probability of voting. In auxiliary regressions where these sophomore-year measures are the dependent variables, the estimated effects of college entrance are positive and statistically significant. However, since the dependent variables in these models preceded college entrance, these results cannot plausibly reflect causal effects. Instead, these results suggest the existence of individual-level unobservables that may have a positive covariance with both educational attainment and adult civic engagement. This stylized evidence suggests the need to rely on instrumental variables in estimating the effects of college attendance on civic outcomes.

Another possible concern with the results in Table 1 involves the quality of the self-reported data on civic engagement. Specifically, prior comparisons of actual voting rates with those based on survey data indicate that survey respondents often overstate their voter turnout. This problem can be particularly acute in survey instruments that focus largely on political values. However, in surveys that emphasize other issues (e.g., the November supplements to the Current Population Surveys), the

aggregate voter-participation rates implied by self reports are generally quite close to the actual rates (Teixeira 1992, Appendix A). Since the HS&B survey instruments asked very few questions about civic values and engagement, the HS&B responses may also track actual behaviors closely. The voter-registration rate implied by the HS&B responses (67 percent) is similar to the contemporaneous CPS-reported rate for 25-34 year olds (61 percent, U.S. Census Bureau 1996). And the percent of HS&B respondents who reported voting in the past year (36 percent) is actually lower than the CPS-reported turnout rate for 25-34 year olds in the 1992 Presidential election (53 percent).[14] However, further comparisons with the CPS data suggest that the HS&B respondents' 1992 recall of having voted in 1988 may be more biased. In the 1988 CPS survey, approximately 38 percent of 21-24 year olds reported voting in the Presidential election while the 1992 HS&B survey suggests that 55 percent of respondents did. So, a caveat about this particular variable is appropriate. However, it is not clear, even in that case, that the estimated marginal effects of educational attainment would be confounded by over-reporting. For example, if schooling promotes more accurate self-reports, these evaluations would only understate the true civic returns to education. On the other hand, it is also possible that educational attainment creates a sense of obligation and guilt that leads more educated respondents to over-state their actual civic engagement more frequently than those with lower educational attainment. However, this does not appear to be the case.[15] Furthermore, even if schooling did increase over-reporting, that would necessarily imply that schooling has a type of structural effect (i.e., instilling a sense of civic obligation) that should also generate true increases in voter turnout.

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[14] That difference is reasonable since nearly all of the HS&B responses occurred before the November 1992 general election.

[15] Specifically, a regression of actual county-level voter turnout in the 1980 election on the county-level percent of adults with a high school degree generates marginal effects that are actually somewhat larger than the estimated effect of being on high school graduate on the voter participation reported by HS&B respondents. It is also somewhat larger than the estimated effect of being a high school graduate on self-reported participation in the 1980 Presidential election among GSS respondents.

Measures of college availability as instruments

The partial correlations reported in Table 1 are consistent with the prior empirical studies of civic participation. However, a more convincing strategy for assessing whether the estimates in Table 1 reflect the causal effects of attending college is to exploit instrumental variables that generate plausibly exogenous variation in this measure of educational attainment. The fundamental requirements of such instrument are that they actually influence educational attainment and that they are uncorrelated with the unobserved determinants of civic engagement. A recent study of the labor-market returns to schooling by Card (1995) suggests that the geographic availability of colleges may provide valid instruments for schooling.[16] The basic motivation for such instruments is that the proximity of colleges as a teen should substantially reduce the costs of attending college (particularly for students from disadvantaged backgrounds) but should otherwise have no effects on adult outcomes. Rouse (1995) also presents evidence that the availability of two-year colleges increases educational attainment for those on the margin of attending college (a "democratization" effect) but actually reduces it among those who would have otherwise attended a four-year college (a "diversion" effect). I also find some support for a modest "diversion" effect (i.e., the proximity of two-year colleges reducing the probability of completing a bachelor's degree) but rely on the stronger "democratization" effect as a source of identifying information.

Specifically, I rely on two measures of the local availability of two-year colleges. One is the distance in miles from each respondent's high school to the nearest two-year college (as reported by a high-school official as part of the HS&B school survey). The second is a count of the number of two-year colleges within each respondent's county in 1983 (mean=2.4).[17] These variables provide

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[16] See Kling (2001) and Currie and Moretti (2002) for further discussions and applications of this approach.

[17] These counts were created using the 1983-84 data from the Higher Education General Information Survey (HEGIS). See the appendix for details. Card (1995) and Kling (2001) rely similarly on a binary indicator for any college in county. I also constructed counts of four-year colleges by county but found that this was highly collinear with the number of two-year colleges and exclude it from this analysis. So, a caveat about attributing the effects associated with this measure to two-year, not 4-year, institutions is appropriate.

related but distinct measures of the availability of two-year colleges. In particular, it is reasonable to suspect that the probability of attending college would be simultaneously influenced by the proximity of the nearest two-year institution as well as by the local availability of several postsecondary options.[18] While every state has two-year colleges, their geographic distribution across the United States is somewhat uneven. For example, several states in the West and Southwest (e.g., California, Washington, Texas and Arizona) and in the upper Midwest (e.g., Illinois, Michigan) have relatively extensive systems of public community colleges. Medsker and Tillery (1971) note that this distribution partly reflects the dramatic mid-century growth in new two-year colleges, which was shaped by the interaction of state-specific enabling legislation and several sources of enrollment pressure (e.g., the G.I. Bill, the baby boom and population migration). However, the states of New York, Pennsylvania, Ohio and Florida also have a large number of two-year colleges, with a particularly large share of them being private junior colleges.

I also considered, but rejected, the idea of using proximity to four-year colleges as an instrument. Specifically, a central concern with any instrument based on the geographic availability of colleges is that it might be flawed because it is associated with the unobserved determinants of both educational attainment and civic behavior. In particular, the unobserved traits of communities near colleges (e.g., high socioeconomic status) could simultaneously encourage both higher educational attainment and increased civic participation. Furthermore, the availability of colleges may promote a youth-oriented and politically aware culture that promotes the civic engagement of teens independently of its effects on educational attainment. I assess the empirical relevance of these concerns in a number of ways. For example, I discuss the robustness of the key results to the introduction of the school, county and state-level controls. However, I also provide three other types of ad-hoc empirical evidence on the validity of these instruments. First, I examine their effects on different levels of educational attainment. If the estimated effects of college availability truly reflect the costs of attending college and not the influence of omitted variables, these instruments are likely

[18] These measures are not highly collinear and have a relatively low correlation coefficient of -0.2.

to have little or no effects on other levels of educational attainment.[19] Second, I examine the partial correlations between the instruments and sophomore-year measures of civic attitudes and knowledge that are strongly correlated with future civic participation (i.e., scores on a civics test and attitudes towards correcting inequality). And, third, I assess how the effects of these instruments vary across students from advantaged and disadvantaged backgrounds. To the extent that the estimated effects of these instruments truly reflect variation in the costs of attending college, these effects should be concentrated among students from disadvantaged backgrounds (Card 1995, Kling 2001). The results of all of these ad-hoc specification checks suggest that the proximity to 4-year colleges may be an invalid instrument. In particular, nearness to 4-year colleges is associated with sharp increases in the probability of graduating from high school as well as significant increases in sophomore-year civics knowledge. These results do not constitute a definitive case against this particular measure as an instrument for educational attainment. Nonetheless, all of the models for educational attainment and civic outcomes reported here condition on this measure.[20]

In Table 2, I present the estimated marginal effects of the availability of two-year colleges on the probability of entering college. The results in the top panel suggest that both measures of availability have plausibly signed and statistically significant effects on college entrance and that these estimates are relatively robust to the introduction of additional controls. Specifically, the results from Model (4) suggest that a location 100 miles further away from a two-year college reduces the probability of college entrance by 6.1 percentage points. Similarly, these results suggest that an additional two-year institution within county is associated with a 0.6 percentage point increase in the probability of entering college. In the bottom panel of Table 2, I provide some ad-hoc evidence on the validity of these instruments by estimating their unique effects on students from advantaged and disadvantaged backgrounds. Card (1995) suggests that, if the interpretation of college availability as

[19] However, a signaling model of education suggests that there could be effects on other levels of attainment (e.g., Lang and Kropp 1986). Also, as noted earlier, these instruments could influence higher levels of attainment through diversion effects (Rouse 1995).
[20] Models that use this measure as an IV return results similar to those reported here.

an independent measure of the costs of attending college is a valid one, the effects of these instruments should be concentrated among students from disadvantaged backgrounds. Following Card (1995), I assess the existence of such response heterogeneity by interacting the availability measures with indicators for high and low parental education.[21] The results in the bottom panel of Table 2 indicate that the effects of the availability of two-year colleges are highly concentrated among students with poorly educated parents. In particular, the estimated effect of the proximity of two-year colleges is statistically insignificant for students with highly educated parents (though it has the same sign). Similarly, the number of two-year colleges within county has a smaller and weakly significant effect for students with highly educated parents.

In Table 3, I present evidence how the availability of two-year colleges influenced different margins of educational attainment. These estimates are based on specifications that include all the individual, family, school, county and state-level controls. The results suggest that the availability of two-year colleges has small and statistically insignificant effects on the probability of graduating from high school and on the probability of obtaining an associate's degree. These results also suggest that the geographic proximity of two-year colleges led to relatively small and weakly significant reductions in the probability of obtaining a bachelor's degree: a "diversion" effect that appears to be concentrated among students from disadvantaged backgrounds. However, the results in the bottom panel of Table 3 also suggest that, for students with poorly educated parents, the number of two-year colleges within county had strong "democratization" effects that increased the probability of entering college as well as the probability of obtaining a bachelor's degree.[22] However, the more general and important result from Table 3 is that the effects associated with the availability of two-year colleges are highly concentrated on the margin of attending college. This evidence is consistent with the

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[21] As in Card (1995), low parental education implies that the highest educational attainment of the parents is high school dropout or missing. Students for whom parents' education is missing have lower levels of attainment than the students who report their parents are dropouts.

[22] These results for attaining a bachelor's degree are not necessarily inconsistent since the proximity of a single institution may promote diversion while the availability of several could facilitate the ultimate progression to a bachelor's degree.

maintained assumption that these measures reflect plausibly exogenous variation in the costs of college entrance and not other unobserved traits of these communities. In particular, because these instruments have such a narrowly focused effect on this single margin of educational attainment, it suggests that they do not proxy for the unobserved determinants of future civic engagement.

However, in Table 4, I present further evidence on the validity of these exclusion restrictions. As noted earlier, the base-year survey of HS&B sophomores contained two variables that appear to reflect each student's latent civic engagement and knowledge well: a standardized test score on questions related to civics and an attitudinal question about the importance of correcting social and economic inequality (1=not important, 2=somewhat important and 3=very important). These latent indicators, which are highly predictive of future civic engagement, provide a potentially plausible basis for evaluating the validity of the instruments. Specifically, if the measures of college availability have an association with the unobserved determinants of future civic engagement, we would expect them to be correlated with these observed measures as well. In Table 4, I present the key results from auxiliary regressions in which these sophomore-year traits are the dependent variables. These results are based on models that include all of the prior controls (e.g., Model (4) in Tables 1 and 2). The estimates in Table 4 uniformly suggest that availability of two-year colleges, both generally and for students with poorly educated parents, has a small and statistically insignificant association with sophomore-year civics knowledge and with community attitudes. Some studies also assess instrument validity and possible biases by considering the sign of the relationship between candidate instruments and observed determinants of the outcomes under study (e.g., Altonji et al. 2002). The mixed signs of the estimates reported in Table 4 do not provide consistent evidence for particular violations of exclusion restrictions. In a similar vein, I also examined the partial correlations between these instruments and the 1980 county-level voter turnout. Interestingly, the results indicated that communities with better access to two-year colleges had lower voter turnout rates. These negative relationships suggest that, if there are violations of the

exclusion restrictions, they may impart a negative bias, which would not be fundamentally confounding for most of the results presented below.

Results

The results in Tables 2, 3 and 4 are consistent with the maintained assumption that the geographic availability of two-year colleges provides a potentially valid source of identification. The availability of two-year colleges is associated with a significant increase in college attendance but smaller and statistically insignificant changes in other measures of educational attainment. These increases are plausibly concentrated among students with poorly educated parents. And these measures are unrelated to sophomore-year indicators of civic attitudes (e.g., civics knowledge and attitudes towards inequality). In Table 5, I present the key results from bivariate probits in which the adult civic behaviors are the dependent variable of interest and college entrance is an endogenous regressor (Wooldridge 2002).[23] The excluded instruments in Model (1) are miles to the nearest two-year college and the number of two-year colleges within county. These results of these models suggest that college entrance has small and imprecisely estimated effects on the probability of volunteering but uniformly large and positive effects on each of the three measures of voter participation. Specifically, these estimates indicate that college entrance increases voter participation by roughly 21 to 30 percentage points. These results are clearly consistent with the conventional claims that educational attainment is a critical determinant of civic engagement. In fact, these estimated effects are substantially larger than those based on partial correlations (Table 1). The sampling variation associated with these estimates suggests that these differences should not be overdrawn. Nonetheless, it is also worth noting three reasons that the true effects of educational

[23] The results of 2SLS estimates generate similarly signed and statistically significant estimates but are substantially larger than the marginal effects and average treatment effects (ATE) based on these bivariate probits, particularly in models saturated with the additional controls. Identification in these bivariate probits appears to be driven by the exclusion restrictions and not by functional form. Bivariate probits that do not rely on excluded instruments generate small and statistically insignificant effects.

attainment might exceed the estimates based on partial correlations (Table 1). First, as frequently noted in the literature on wages and schooling, this could reflect an attenuation bias driven by measurement error in reported schooling. Second, these estimates could indicate that the civic returns associated with college entrance are particularly large for the non-random subset of individuals whose post-secondary attainments were influenced by the instruments (e.g., those from disadvantaged backgrounds, Imbens and Angrist 1994). And, third, a downward bias in conventional estimates could also reflect the influence of unobserved ability on both schooling decisions and time allocated to civic endeavors.

However, a fourth possibility with very different implications is that the size of these estimates reflects undiagnosed violations of the maintained exclusion restrictions. One indication that this is not so is that the results from Table 5 are quite similar across models, which incrementally introduce the school, county and state-level controls. However, another way to assess this concern is to use as instruments the interaction of low parental education and the measures of two-year college availability. Specifically, in such models, the interaction of high parental education and the measures of two-year college availability can then be included as controls in the outcome equations (e.g., Card 1995). This approach to identification can provide effective controls for the possible, indirect effects on civic outcomes associated with college availability to the extent that these effects are constant across students with different family backgrounds. The results based on this specification are reported in the right panel of Table 5 (i.e., Model (2)) and are quite similar to those based on the basic instruments. The robustness of these results suggests that the basic identifying assumptions are accurate. I also assessed this issue by relying alternatively on miles to a two-year college and number of two-year colleges in county as the sole instrument and including the other variable as a control. This approach leads to similarly large and positive point estimates in models for voter registration and turnout. However, in most cases, the reduction in identifying assumptions makes these estimates statistically imprecise.

SECONDARY SCHOOLING AND CIVIC OUTCOMES

General Social Surveys (GSS)

The evidence from the HS&B data has at least two critical shortcomings. One is that it only identifies the civic returns to education at the post-secondary level. And the second is that the available data provide no measures of the degree of civic awareness or of other fundamental civic values. The data from the General Social Surveys (GSS) provide an opportunity to address both of these concerns. The GSS is a nationwide survey, conducted every one to two years, on a broad range of attitudes and behavior.[24] My extract is based on the pooled 1972-2000 surveys and consists of the respondents who lived in the U.S. at age 16 and were 14 years old between 1914 and 1978. In each survey, these respondents were asked about their educational attainment and whether they voted in the last Presidential election (mean=.73).[25] In most, but not all, survey years, GSS respondents were also asked about how often they read the newspaper, about their group memberships (e.g., fraternal and community-service groups, political clubs, school-service and youth groups, church-service groups, etc.) and about their attitudes towards free speech for particular groups.

The GSS respondents report an average of 1.8 group memberships. The frequency of newspaper readership is based on five possible responses (never, less than once a week, once a week, a few times a week and every day) coded here as varying from 0 to 4 (mean=3.2). This measure of newspaper readership is meant to indicate whether voters stay informed about current affairs. There are inarguably better ways of measuring the degree of civic awareness. For example, in 1987, the GSS respondents were asked to identify their congressman. Interestingly, only 37 percent of respondents were able to answer this question correctly. Unfortunately, since this question was only asked in 1987, there are relatively few observations (n=1,555) and a plausible identification strategy

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[24] See the appendix for details on the GSS and construction of this extract.

[25] Actual votes cast as a percentage of the voting-age population declined from 61 to 49 percent over these eight elections. These observed turnout rates should be somewhat lower than the GSS-reported rates since the voting-age population includes ineligible voters. However, the GSS respondents may overstate their actual voter participation more dramatically in the most recent survey years. In Table 10, I present evidence on the implications for this study's key inferences of this possible change in reporting biases.

cannot be implemented. However, the data from 1987 do indicate that the frequency of newspaper readership is strongly associated with being able to identify your congressman. Specifically, conditional on all the covariates discussed below, a one-unit increase in the measure of newspaper readership is associated with a 10 percentage-point increase in the probability of answering correctly (i.e., a 27-percent increase in the mean). This suggests that the frequency of newspaper readership is a reasonable proxy for the degree of civic awareness. The measures of attitudes towards free speech are based on separate survey questions that allowed respondents to indicate whether they would allow particular types of people to speak in their community. These types include someone against churches and religion (an anti-religionist), an admitted Communist, an admitted homosexual, someone who advocates outlawing elections and letting the military run the country (a militarist) and someone who believes blacks are inferior (a racist). Support for allowing free speech ranges from 59 percent for the militarist to 73 percent for the homosexual (see appendix Table 2).

Baseline estimates

In Table 6, I present baseline OLS estimates of how years of completed schooling influences these measures of civic engagement and attitudes. The sparsest specification only includes as controls basic demographic information (9 variables) and fixed effects for survey year (as many as 22 variables), year of birth (64 variables) and Census division of residence at age 16 (8 variables). The second specification adds three control variables that reflect the quality of public schools and the degree of civic engagement in each respondent's teen community. The two school quality measures are the pupil-teacher ratios and relative teacher salaries in public schools at age 14 in the Census division of residence at age 16.[26] The third variable is the voter turnout in the Presidential election that occurred between the ages of 13 and 16 in the Census division of residence at age 16. The third

[26] Card and Krueger (1992) present evidence that these measures influenced average years of schooling. I converted average teacher salaries to a relative measure by exploiting data on wages paid to road workers on Federal projects (Card and Krueger 1992) and data on wages for production workers in manufacturing. See the appendix for information on the construction of these variables.

specification introduces variables based on survey responses that reflect a variety of family and community-specific traits. These include family income at age 16 (5 variables), family structure at age 16 (5 variables), parental education (4 variables) and the urbanicity of residence at age 16 (6 variables). In the final model, I control for all the unobserved determinants that might be specific to a particular Census division in a particular year (e.g., weather, close political races, etc.) by including approximately 200 fixed effects for each unique Census-division and survey-year combination. The standard errors are adjusted for unspecified heteroscedasticity specific to the Census division of resident at age 16.[27] The results in Table 6 uniformly indicate that schooling is strongly and positively correlated with all of these measures of civic engagement and attitudes. For example, these estimates suggest that an additional year of schooling increases voter participation by 3.8 percent, an increase of approximately 5 percent. These results also imply that another year of schooling significantly increases the index of newspaper readership (by 0.104, an increase of 3 percent) and the number of group memberships (by .222, an increase of 12 percent). Another year of schooling also appears to increase support for free speech by a statistically significant 2.2 to 3.6 percentage points, depending on who is doing the speaking. Interestingly, these estimated effects are generally quite robust to dramatic increases in the set of controls for observed traits.

Restrictive child labor laws as an instrument

The estimates in Table 6 suggest that additional years of schooling led to significant increases in the quality and quantity of civic engagement and in the support for free speech. I attempt to assess whether these estimates reflect a causal relationship by exploiting the exogenous variation in years of schooling generated by teen exposure to changes in child labor laws. Recent studies by Acemoglu and Angrist (2000) and Lleras-Muney (2002) provide evidence that the variation in child

[27] This conservative approach may be appropriate since the pre/post nature of the instrumental variable and serial correlation in the dependent variables could lead to overstated precision (Bertrand et al. 2002). As a practical matter, this only appears to increase the 2SLS standard errors slightly. However, this approach is also a conservative one because there are only 8 degrees of freedom in the critical value of the t-statistic since there are only 9 Census divisions.

labor laws influenced the amount of schooling at the secondary level. In Tables 7 and 8, I present evidence on how these variables influenced educational attainment among the GSS respondents. The coding of these child labor laws are discussed in detail in Acemoglu and Angrist (2000). Essentially, for each state and year from 1914 to 1978, they identified the minimum amount of schooling required before a child could enter the workforce (the variable CL). This variable is equal to the greater of the years of schooling a state required before granting a work permit and the difference between the age at which children could work and the age at which they had to enter school. These laws are represented here by a dummy variable equal to one for CL greater than or equal to 9. These state-year laws could not be matched directly to GSS respondents because the available data only identifies which of 9 Census divisions they resided in at age 16. Therefore, I calculated division-by-year means of these state-year dummies using state-year population estimates as weights.[28] I then matched each GSS respondent to these fractional variables representing restrictive child-labor laws that were in effect at age 14 in their reported division of residence at age 16.

In Table 7, I present evidence on how this variable influenced years of completed schooling among the full group of respondents for whom voting data are available as well as among the smaller samples who were asked questions about newspaper readership, group memberships and free speech. The sparsest specification (Model (1)) suggests that exposure to a restrictive child labor law increased years of schooling by roughly one year. These estimates are somewhat sensitive to introducing the contemporaneous measures of school quality and civic engagement as controls (Model (2)). However, the results from the remaining specifications are largely unchanged after introducing the family/community controls and fixed effects specific to each division and survey-year cell. Specifically, these estimates suggest that teen exposure to restrictive child labor laws

[28] This construction introduces measurement error into the instruments. However, it is not clear that the implied measurement error is any less than that in other studies based on PUMS data, which identify state of birth but not state of teen residence.

increased years of schooling by a statistically significant .5 to .7 years.[29] The quality of this measure of restrictive child-labor laws as an instrument hinges critically on the maintained assumption that these estimates accurately reflect its independent effects on educational attainment. The evidence from prior studies is generally consistent with this view. For example, Lleras-Muney (2002) presents a variety of ad-hoc empirical evidence on changes in child-labor laws and concludes that they were not endogenously determined. Furthermore, Goldin (2001) argues that such laws played a relatively minor role in the dramatic "high school movement" from 1910 to 1940, which suggests that these law changes were not part of substantive social changes that might have also influenced civic attitudes..

The robustness of the first-stage estimates in Table 7 to the introduction of the additional controls (i.e., across Models (2), (3) and (4)) also provides supporting evidence. However, in Table 8, I provide additional empirical evidence on the validity of these instruments by assessing some straightforward counterfactuals. More specifically, if these models effectively identify the influence of stricter child-labor laws on educational attainment, we should find that these estimated effects are largely concentrated at the lower end of the distribution of educational attainment (Acemoglu and Angrist 2000, Lleras-Muney 2002). However, we should be especially concerned about the existence of undiagnosed specification errors if similarly specified models indicate that these laws had substantive effects on higher levels of educational attainment. In particular, that could indicate that the within-division variation in strict child-labor laws had a confounding correlation with the unobserved determinants of educational attainment and other dimensions of youth development. In Table 8, I present the estimated effects of restrictive child-labor laws on different levels of educational attainment based on specifications that include the full set of controls (i.e., as in Model (4) in Tables 6 and 7). These estimates indicate that the effects associated with stricter child-labor

[29] These estimated effects are somewhat larger than those reported by Acemoglu and Angrist (2000). However, these differences appear to reflect the unique composition of their sample. They consider older white males from particular birth cohorts. I get similar point estimates for the relevant sub-sample of GSS respondents.

laws were largely concentrated at the secondary level. More specifically, these estimates indicate that the strictest child-labor laws led to large and statistically significant increases in the probability of completing 9, 10, 11 and 12 years of schooling and the probability of high school graduation. However, the same specifications indicate that these law changes had smaller and statistically insignificant effects on several measures of post-secondary educational attainment.

Results

The results in Tables 7 and 8 suggest that restrictive child labor laws may provide a valid source of identifying information for estimating effects of variation in secondary schooling on adult civic behaviors. In Table 9, I present 2SLS estimates of the effect of years of completed on each measure of civic engagement.[30] The results indicate that schooling has uniformly positive and statistically significant effects on most measures of civic engagement and attitudes. For example, these 2SLS estimates suggest that an additional year of schooling increased voter participation by a weakly significant 6.8 percentage points (t-statistic=1.93), which is nearly twice the change implied by the OLS estimate. The estimates in Table 9 also suggest that schooling increases the quality of civic engagement and knowledge. More specifically, the 2SLS estimates imply that an additional year of schooling generates a weakly significant increase (t-statistic=2.02) in the frequency of newspaper readership that is roughly equivalent to that implied by the OLS estimate. The estimated effect of schooling on group memberships is also positive but highly imprecise. However, these estimates also imply that that schooling significantly increased support for free speech by anti-religionists, communists and homosexuals. These estimated effects (8.0 to 12.5 percentage points) are several times larger than those implied by the corresponding OLS estimates. But the estimated effects of schooling on support for speech by militarists and racists are smaller and statistically imprecise.

[30] I also experimented with specifications that recognized the categorical nature of the dependent variable and the potential endogeneity of schooling (Wooldridge 2002) and found that they generated similar results.

One of the concerns noted earlier is that the self-reported data on voting participation may overstate actual turnout more dramatically in the most recent GSS surveys (particularly for survey responses regarding the 1996 Presidential election). In Table 10, I assess whether a possible change in reporting biases may have influenced this study's key inferences. This evidence is based on the 2SLS estimates from models that incrementally exclude data from the more recent GSS surveys. The results suggest that recent changes in reporting biases may impart a downward bias to the estimated effect of schooling on voter turnout. Specifically, in models that exclude the most recent surveys, the estimated effect of schooling on voter participation is nearly twice as large. While this sensitivity could be due to any number of factors (e.g., cohort-specific changes in the schooling-voting relationship), it is also consistent with an increased trend towards overstating voter participation among less-educated respondents. Regardless, these results suggest that the estimates in Table 9 could be understood as a lower bound for the effect of secondary schooling on voter participation.

CONCLUSIONS

In this study, I presented an empirical analysis of one of the fundamental relationships that motivates public policies towards education: the effects of schooling on civic participation and attitudes. In particular, I assessed whether increases in educational attainment have causal effects on civic outcomes by exploiting possibly exogenous sources of variation in schooling that should otherwise be unrelated to civic outcomes in adulthood (i.e., the geographic availability of two-year colleges as a teen and exposure to child labor laws as a teen). The results suggested that educational attainment, both at the post-secondary and the secondary levels, has large and independent effects on most measures of civic engagement and attitudes. The apparent existence of these civic returns implies that much of the long-lived hyperbole about the important role of education in a functioning democracy may be accurate. However, it should also be noted that a great deal of the discussion surrounding the role of education in a democracy has also confused the existence of these externalities with other fundamental issues related to how the government should intervene in the

market for education (e.g., price subsidies, regulation of the private sector, public production). In particular, the existence of large civic returns to education is not necessarily relevant to the difficult question of whether government should be involved in directly producing education (i.e., the "choice of instrument" problem, Poterba 1996). Nonetheless, these results clearly underscore the dramatic relevance of schooling to the critical functions of a democratic society and imply that initiatives to promote educational attainment merit the continued and careful scrutiny of researchers and policymakers.

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Table 1 - Estimated Marginal Effects of College Entrance on Adult Civic Behaviors, Single-Equation Probits, HS&B

Dependent Variable	(1)	(2)	(3)	(4)	Sample Size
Registered to vote	.129‡	.129‡	.131‡	.130‡	11,366
	(.011)	(.011)	(.011)	(.011)	
Voted in last 12 months	.102‡	.101‡	.105‡	.103‡	11,429
	(.011)	(.011)	(.011)	(.011)	
Voted in 1988 Presidential election	.178‡	.177‡	.180‡	.179‡	11,370
	(.011)	(.011)	(.011)	(.011)	
Volunteered in last 12 months	.073‡	.075‡	.074‡	.074‡	11,484
	(.010)	(.010)	(.010)	(.010)	
School-level controls	no	yes	yes	yes	
State/county-level controls	no	no	yes	yes	
Census division dummies	no	no	no	yes	

All models include binary indicators for gender (1), age (1), race/ethnicity (3), religious affiliation (5), family income (8), parental education (4) and family composition (5). Standard errors, adjusted for clustering at the school level, are reported in parentheses.

* Statistically significant at the 10-percent level

† Statistically significant at the 5-percent level

‡ Statistically significant at the 1-percent level

Table 2 - Estimated Marginal Effects of College Availability on College Entrance, Single-Equation Probits, HS&B

Independent Variable	(1)	(2)	(3)	(4)
Miles to a two-year College (÷100)	-.097‡	-.077‡	-.059†	-.061†
	(.025)	(.024)	(.024)	(.024)
Number of two-year Colleges in County	.008‡	.007‡	.006‡	.006‡
	(.001)	(.001)	(.001)	(.002)
Log Likelihood	-6,348	-6,336	-6,316	-6,309
Low Parental Education x Miles to a two-year College (÷100)	-.155‡	-.135‡	-.113‡	-.118‡
	(.035)	(.034)	(.033)	(.034)
High Parental Education x Miles to a two-year College (÷100)	-.053	-.035	-.017	-.018
	(.033)	(.033)	(.033)	(.033)
Low Parental Education x Number of two-year Colleges in County	.010‡	.008‡	.008‡	.008‡
	(.002)	(.002)	(.002)	(.002)
High Parental Education x Number of two-year Colleges in County	.006‡	.005†	.005†	.004*
	(.002)	(.002)	(.002)	(.002)
Log Likelihood	-6,343	-6,331	-6,311	-6,304
School-level controls	no	yes	yes	yes
State/county-level controls	no	no	yes	yes
Census division dummies	no	no	no	yes

The sample size is 11,489. All models include binary indicators for gender (1), age (1), race/ethnicity (3), religious affiliation (5), family income (8), parental education (4) and family composition (5). Standard errors, adjusted for clustering at the school level, are reported in parentheses.

* Statistically significant at the 10-percent level
† Statistically significant at the 5-percent level
‡ Statistically significant at the 1-percent level

Table 3 - Estimated Marginal Effects of College Availability on Educational Attainment, Single-Equation Probits, HS&B

Independent Variable	Dependent Variable			
	High School Graduate	College Entrant	Associate's Degree	Bachelor's Degree
Model (1)				
Miles to a two-year College (÷100)	-.001 (.011)	-.061† (.024)	.022 (.021)	.036* (.020)
Number of two-year Colleges in County	.0005 (.001)	.006‡ (.002)	.002 (.002)	.003 (.002)
Model (2)				
Low Parental Education x Miles to a two-year College (÷100)	.006 (.014)	-.118‡ (.034)	.019 (.028)	.056† (.023)
High Parental Education x Miles to a two-year College (÷100)	-.009 (.014)	-.018 (.033)	.024 (.026)	.025 (.027)
Low Parental Education x Number of two-year Colleges in County	.0005 (.001)	.008‡ (.002)	.002 (.002)	.006‡ (.002)
High Parental Education x Number of two-year Colleges in County	.0004 (.001)	.004* (.002)	.001 (.002)	.001 (.002)
Sample Size	11,475	11,489	11,343	11,343

All models include binary indicators for gender (1), age (1), race/ethnicity (3), religious affiliation (5), family income (8), parental education (4) and family composition (5), school-level controls (3), state/county-level controls (5) and Census division dummies (8). Standard errors, adjusted for clustering at the school level, are reported in parentheses.

* Statistically significant at the 10-percent level

† Statistically significant at the 5-percent level

‡ Statistically significant at the 1-percent level

Table 4 – OLS-Estimated Effects of College Availability
On 1980 Civics-Related Variables, HS&B

Independent Variable	Dependent Variable: Civics test score	Dependent Variable: Importance of Correcting Inequality
Model (1)		
Miles to a two-year College (÷100)	.034 (.640)	-.021 (.029)
Number of two-year Colleges in County	-.006 (.030)	.002 (.002)
Model (2)		
Low Parental Education x Miles to a two-year College (÷100)	.419 (.578)	-.045 (.039)
High Parental Education x Miles to a two-year College (÷100)	-.285 (.875)	-.001 (.041)
Low Parental Education x Number of two-year Colleges in County	.020 (.040)	.001 (.003)
High Parental Education x Number of two-year Colleges in County	-.023 (.036)	.003 (.002)
Sample Size	9,751	10,336

All models include binary indicators for gender (1), age (1), race/ethnicity (3), religious affiliation (5), family income (8), parental education (4) and family composition (5), school-level controls (3), state/county-level controls (5) and Census division dummies (8). Standard errors, adjusted for clustering at the school level, are reported in parentheses.

* Statistically significant at the 10-percent level
† Statistically significant at the 5-percent level
‡ Statistically significant at the 1-percent level

Table 5 – Estimated Effects of College Entrance on Adult Civic Behaviors, Bivariate Probits, HS&B

Dependent variable	Model (1)			Model (2)			Sample Size
	$\hat{\beta}$	Marginal Effect	ATE	$\hat{\beta}$	Marginal Effect	ATE	
Registered to vote	.791‡ (.282)	.283	.280	.793‡ (.263)	.284	.280	11,366
Voted in last 12 months	.579† (.233)	.210	.208	.552† (.217)	.201	.198	11,429
Voted in 1988 Presidential election	.793‡ (.273)	.307	.300	.795‡ (.254)	.307	.301	11,366
Volunteered in last 12 months	.028 (.271)	.010	.010	.059 (.270)	.022	.022	11,484

All models include binary indicators for gender (1), age (1), race/ethnicity (3), religious affiliation (5), family income (8), parental education (4) and family composition (5), school-level controls (3), state/county-level controls (5) and Census division dummies (8). The two instrumental variables for Model (1) are miles to a two-year college and the number of two-year colleges in county. The two instrumental variables for Model (2) are interactions of low parental education with miles to a two-year college and with the number of two-year colleges in county. Model (2) also includes as controls interactions of high parental education with miles to a two-year college and with the number of two-year counties in county. Standard errors, adjusted for clustering at the school level, are reported in parentheses.

* Statistically significant at the 10-percent level
† Statistically significant at the 5-percent level
‡ Statistically significant at the 1-percent level

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Table 6 – OLS Estimates of the Effect of Highest Grade Completed on Civic Behaviors & Attitudes, 1972-2000 GSS

Dependent Variable	(1)	(2)	(3)	(4)	Sample Size
Voted in last Presidential election	.043‡ (.002)	.038‡ (.002)	.038‡ (.002)	.038‡ (.002)	32,111
Newspaper readership	.112‡ (.013)	.104‡ (.014)	.105‡ (.014)	.104‡ (.014)	21,805
Group memberships	.239‡ (.009)	.221‡ (.011)	.222‡ (.011)	.222‡ (.011)	16,361
Allow anti-religionist to speak	.036‡ (.001)	.030‡ (.001)	.030‡ (.001)	.029‡ (.001)	22,449
Allow communist to speak	.043‡ (.002)	.036‡ (.002)	.036‡ (.002)	.036‡ (.002)	22,111
Allow homosexual to speak	.035‡ (.002)	.029‡ (.001)	.029‡ (.001)	.029‡ (.001)	20,678
Allow militarist to speak	.037‡ (.002)	.031‡ (.002)	.030‡ (.002)	.030‡ (.002)	18,514
Allow racist to speak	.027‡ (.002)	.023‡ (.002)	.022‡ (.002)	.022‡ (.002)	18,488
Teen-division/cohort controls	no	yes	yes	yes	
Family/community controls	no	no	yes	yes	
Current-division-by-survey-year dummies	no	no	no	yes	

All models include age, age squared and binary indicators for gender (1), race (2) and religious preference (4) and fixed effects for survey year, year of birth and Census division of residence at age 16. Standard errors, adjusted for clustering at the division level, are reported in parentheses.

* Statistically significant at the 10-percent level
† Statistically significant at the 5-percent level
‡ Statistically significant at the 1-percent level

Table 7 – OLS Estimates of the Effects of Restrictive Child-Labor Laws on Highest Grade Completed, 1972-2000 GSS

Sample	(1)	(2)	(3)	(4)
Voting sample (n=32,111)	.97‡ (.15)	.56‡ (.13)	.59‡ (.15)	.53‡ (.14)
Newspaper sample (n=21,805)	1.01‡ (.18)	.52‡ (.16)	.57† (.20)	.56‡ (.18)
Group membership sample (n=16,361)	1.14‡ (.18)	.72‡ (.14)	.63‡ (.14)	.54‡ (.12)
Free speech sample (n=18,488)	1.13‡ (.13)	.81‡ (.18)	.80‡ (.16)	.70‡ (.14)
Teen-division/cohort controls	no	yes	yes	yes
Family/community controls	no	no	yes	yes
Current-division-by-survey-year dummies	no	no	no	yes

All models include age, age squared and binary indicators for gender (1), race (2) and religious preference (4) and fixed effects for survey year, year of birth and Census division of residence at age 16. Standard errors, adjusted for clustering at the division level, are reported in parentheses.

* Statistically significant at the 10-percent level
† Statistically significant at the 5-percent level
‡ Statistically significant at the 1-percent level

Table 8 – OLS Estimates of the Effects of Restrictive Child-Labor Laws on Measures of Educational Attainment, 1972-2000 GSS

Dependent Variable	$\hat{\beta}$
Highest grade completed	.53‡
	(.14)
Completed 9th grade or higher	.12‡
	(.02)
Completed 10th grade or higher	.10‡
	(.01)
Completed 11th grade or higher	.10‡
	(.02)
Completed 12th grade or higher	.10‡
	(.01)
High school graduate	.08‡
	(.02)
Completed at least 1 year of college	-.04
	(.03)
Associate's degree	-.03
	(.02)
Bachelor's degree	-.03
	(.02)

The sample size is 32,111. All models include age, age squared, binary indicators for gender (1), race (2), religious preference (4), family income at age 16 (5), parental education (4) and family composition at age 16 (5), urbanicity of residence at age 16 (6), pupil-teacher ratio at age 14, relative teacher salaries at age 14, voter turnout as a teen and fixed effects for year of birth, Census division of residence at age 16 and current division-of-residence by survey-year dummies. Standard errors, adjusted for clustering at the division level, are reported in parentheses.

* Statistically significant at the 10-percent level
† Statistically significant at the 5-percent level
‡ Statistically significant at the 1-percent level

Table 9 – 2SLS Estimates of the Effect of Highest Grade Completed on Civic Behaviors & Attitudes, 1972-2000 GSS

Dependent Variable	(1)	(2)	(3)	(4)	Sample Size
Voted in last Presidential election	.037*	.064	.069*	.068*	32,111
	(.019)	(.036)	(.036)	(.035)	
Newspaper readership	.203‡	.127*	.112*	.113*	21,805
	(.021)	(.066)	(.056)	(.056)	
Group memberships	.138†	.144	.157	.164	16,361
	(.047)	(.108)	(.141)	(.184)	
Allow anti-religionist to speak	.092‡	.137‡	.126‡	.125‡	22,449
	(.032)	(.028)	(.024)	(.021)	
Allow communist to speak	.060†	.085‡	.084‡	.080‡	22,111
	(.026)	(.019)	(.020)	(.021)	
Allow homosexual to speak	.092‡	.138‡	.126‡	.123‡	20,678
	(.018)	(.026)	(.021)	(.028)	
Allow militarist to speak	.005	.054†	.053*	.036	18,514
	(.027)	(.023)	(.025)	(.031)	
Allow racist to speak	.040	.022	.020	-.002	18,488
	(.031)	(.032)	(.031)	(.032)	
Teen-division/cohort controls	no	yes	yes	yes	
Family/community controls	no	no	yes	yes	
Current-division-by-survey-year dummies	no	no	no	yes	

All models include age, age squared, binary indicators for gender (1), race (2), religious preference (4) and fixed effects for survey year, year of birth and Census division of residence at age 16. Standard errors, adjusted for clustering at the division level, are reported in parentheses.

* Statistically significant at the 10-percent level

† Statistically significant at the 5-percent level

‡ Statistically significant at the 1-percent level

Table 10 – 2SLS Estimates of the Effect of Highest Grade Completed on Voter Participation, 1972-2000 GSS

Survey Years	$\hat{\beta}$	Sample Size
1970-2000	.068* (.035)	32,111
1970-1996	.103* (.049)	28,533
1970-1994	.102* (.049)	26,540
1970-1993	.118† (.050)	24,254
1970-1991	.132† (.048)	23,013
1970-1990	.134† (.051)	21,813
1970-1989	.152† (.062)	20,693
1970-1988	.155† (.064)	19,425
1970-1987	.162† (.068)	18,216

All models include age, age squared, binary indicators for gender (1), race (2), religious preference (4), family income at age 16 (5), parental education (4) and family composition at age 16 (5), urbanicity of residence at age 16 (6), pupil-teacher ratio at age 14, relative teacher salaries at age 14, voter turnout as a teen and fixed effects for year of birth, Census division of residence at age 16 and current division-of-residence by survey-year dummies. Standard errors, adjusted for clustering at the division level, are reported in parentheses.

* Statistically significant at the 10-percent level
† Statistically significant at the 5-percent level
‡ Statistically significant at the 1-percent level

DATA APPENDIX

A. High School and Beyond (HS&B) Sophomore Cohort

HS&B, one of the U.S. Department of Education's major longitudinal studies (Ingels and Baldridge 1995), began with 1980 high school sophomores and seniors. The base-year samples were based on a two-stage, stratified, probability design. In the first stage, high schools were chosen. Certain types of schools (e.g., those with large Hispanic enrollments, Catholic schools with large minority enrollments) were oversampled (Zahs et al. 1995). In the second stage, as many as 36 sophomores were randomly chosen from participating schools. The initial HS&B sample included over 30,000 high school sophomores from 1,105 schools. Follow-up interviews of a stratified sample of the original sophomore cohort occurred in 1982, 1984, 1986 and 1992. This study is based on the 12,022 respondents from the sophomore cohort who participated in the second (1984) and fourth (1992) follow-up interviews. The interviews for the fourth follow-up occurred from February 1992 through January 1993. Some observations were deleted because they were missing data on post-secondary attainment in the 2nd follow-up (n=108) and because they attended a base-year school for which the school survey responses did not provide information on college proximity (n=425). Of the remaining 11,489 respondents, almost all answered the 4th follow-up questions on voting and volunteering (see Table A1). The extract includes basic information on the gender, race/ethnicity, age, and religious affiliation of the respondents. This extract also includes base-year information for each respondent on family income (9 categories), highest parental education (5 categories), family structure (6 categories) and the urbanicity of the high school area (3 categories). The 1980 attitudinal question on the importance of correcting social/economic inequality has three possible responses: not important (1), somewhat important (2) and very important (3). These respondents were also matched to 1980 county-level data on voter turnout, on the percent of the population aged 18-24 and on the percent of adults who graduated from high school, which were drawn from ICPSR study number 8314. Data on two-year colleges (institution type of 3, 5 or 6) were drawn from the 1983-1984 Higher Education General Information Survey (HEGIS, ICPSR study number 8291). County-level counts were generated after excluding central offices and institutions that are for-profit or require graduation from a two-year or 4-year college for admittance. Data on state-level voter regulations in 1992 were taken from Knack (1995, Appendix B).

Table A1 - HS&B Variables and Means

Variables (Survey Year)	Mean	Sample Size
Currently registered to vote (1992)	.669	11,366
Voted in past 12 months (1992)	.355	11,429
Vote in 1988 Presidential election (1992)	.553	11,370
Any volunteer work in last 12 months (1992)	.371	11,484
High school graduate (1984)	.844	11,475
College entrant (1984)	.543	11,489
Importance of correcting inequality (1980)	1.8	10,336
Civics standardized test score (1980)	50.8	9,751
Female	.521	11,489
Black	.124	11,489
Hispanic	.209	11,489
Other Race	.051	11,489
Older (Born Before 1964)	.284	11,489
Protestant	.332	11,489
Catholic	.382	11,489
Other Christian	.047	11,489

Jewish	.011	11,489
Other Religion	.037	11,489
Religious background: none/missing	.133	11,489
Family income missing	.214	11,489
Family income <$8,000	.060	11,489
Family income $8,000 to $14,999	.117	11,489
Family income $15,000 to $19,999	.105	11,489
Family income $20,000 to $24,999	.109	11,489
Family income $25,000 to $29,999	.106	11,489
Family income $30,000 to $39,999	.127	11,489
Family income $40,000 to $49,999	.071	11,489
Family income $50,000 or higher	.092	11,489
Parent education missing	.162	11,489
Parent high school dropout	.282	11,489
Parent high school graduate	.197	11,489
Parent some college	.212	11,489
Parent college graduate	.148	11,489
Single mother	.136	11,489
Single father	.027	11,489
Natural mother/stepfather	.057	11,489
Natural father/stepmother	.015	11,489
Other family structure	.099	11,489
Both parents	.666	11,489
School-level variables		
Urban school	.227	11,489
Suburban school	.503	11,489
Rural school	.270	11,489
Miles to a 4-year college (÷100)	.167	11,489
Miles to a two-year college (÷100)	.167	11,489
State/county-level variables		
Number of two-year colleges in county	2.43	11,489
1980 county-level votes for President ÷ 18+ population	.529	11,489
1980 county-level population aged 18 to 24	.529	11,489
1980 county-level percent high school graduates among 25+ population	.660	11,489
1992 state-level active mail-in voter registration	.474	11,489
1992 state-level years with "motor-voter" regulations	1.4	11,489

B. 1972-2000 General Social Surveys (GSS)

The GSS is a personal-interview survey conducted every one to two years since 1972 and designed to track a broad range of social attitudes and behaviors over time (NORC 2001). These surveys were based on multi-stage probability samples of English-speaking persons aged 18 and over living in non-institutional settings. The structure of the sampling design was broadly consistent over time. The primary sampling units were generally Standard Metropolitan Statistical Areas (SMSA), counties and independent cities. In the second stage, block groups and enumeration districts were chosen. In block groups and enumeration districts with large numbers of dwellings, a third stage was sometimes carried out to select dwellings within a block. One interview was conducted at each selected house. The 1972-2000 cumulative data file consists of 40,933 respondents (ICPSR study no. 3197). I deleted respondents aged 22 or less (n=2,810), those aged 14 in 1913 or earlier (n=668),

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those aged 14 in 1979 or later (n=2,892), those not in the U.S. at age 16 (n=1,709) and those missing data on educational attainment (n=78). The remaining sample consisted of 32,776 observations. Each GSS survey asked about voter participation in the most recent Presidential election (1968-1996). Most of the limited non-response to the voting questions is due to respondents identifying themselves as ineligible to vote. In most survey years (all but 1973, 1974, 1976, 1980, 1984), respondents were also asked how often they read the newspaper: never (0), less than once a week (1), once a week (2), a few times a week (3), every day (4). In all years except for 1972, 1973, 1976, 1982, 1985, 1996, 1998 and 2000, respondents were also asked about their membership in various types of groups and organizations. Except for the surveys in 1975, 1978, 1983 and 1986, respondents were also asked about allowing free speech for anti-religionists, communists and homosexuals (also not asked in 1972). The questions about free speech for militarists and racists were asked in all survey years except 1972-1975, 1978, 1983 and 1986. The sample means and sample sizes for the voting, newspaper, group-membership and free speech variables are reported in Table A2. In addition to the basic demographic information (age, gender, race, religious preference), these respondents also provided retrospective information on their Census division of residence at age 16 (9 categories), their family income at age 16 (6 categories), family structure at age 16 (6 categories), the urbanicity of their residence at age 16 (7 categories) and their parent's highest educational degree (5 categories). Using state-year data on whether restrictive child labor laws were in effect (CL of 9 or higher; Acemoglu and Angrist 2000), I calculated a population-weighted variable by year and Census division. I then matched the GSS respondents to the law variable in effect when they were aged 14 in their reported Census division of residence at age 16. I also matched the respondents to the pupil-teacher ratios and relative teacher salary at age 14 in the public schools in their Census division of residence at age 16. The sources for enrollment and teacher data were various editions of the *Biennial Survey of Education*, the *Statistical Abstract of the United States* and the *Digest of Education Statistics*. I interpolated annual values for these series when they were only available every other year. Another source of measurement error in these data is that, in a small number of years, some states combined other instructional staff (e.g., librarians) with teacher counts and salaries. I calculated relative teacher salaries by using division-year data on wages paid to road workers on Federal projects (1914-1956; Card and Krueger 1992) and wages paid to production workers in manufacturing (1957-1978). I also matched respondents to the voter turnout in Presidential elections that occurred in their division of residence between the ages of 13 and 16. For example, respondents aged 14 between 1914 and 1917 were matched to the voter turnout in 1916 for their Census division of residence at age 16. The data on voter turnout were drawn from various editions of the *Statistical Abstract of the United States*. For elections before 1952, estimates of the voting-age population are based on the nearest decennial Census. For the voting-age population during the 1916 election, I used one-half of the 1920 estimate.

Table A2 - 1972-2000 GSS Variables and Means

Variables	Mean	Sample Size
Voted in last Presidential election	.73	32,111
Newspaper readership (0 to 4)	3.1	21,805
Group memberships	1.8	16,361
Allow anti-religionists to speak	.69	22,449
Allow Communists to speak	.61	22,111
Allow homosexuals to speak	.73	20,678
Allow militarists to speak	.59	18,514
Allow racist to speak	.62	18,488
Highest grade completed	12.5	32,776
Female	.56	32,776

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Black	.13	32,776
Other Race	.02	32,776
Age	48	32,776
Religious preference: Protestant	.65	32,776
Religious preference: Catholic	.24	32,776
Religious preference: Jewish	.02	32,776
Religious preference: none/missing	.08	32,776
Religious preference: other religion	.02	32,776
Family/Community variables		
Family income at 16: Far below average	.06	32,776
Family income at 16: Below average	.20	32,776
Family income at 16: Average	.41	32,776
Family income at 16: Above average	.11	32,776
Family income at 16: Far above average	.01	32,776
Family income at 16: Missing/unknown	.22	32,776
Family structure at 16: Father/mother	.75	32,776
Family structure at 16: Father/step-mother	.02	32,776
Family structure at 16: Step-father/mother	.04	32,776
Family structure at 16: Single father	.02	32,776
Family structure at 16: Single mother	.11	32,776
Family structure at 16: Other/missing	.06	32,776
Residence at 16: Open country/not farm	.11	32,776
Residence at 16: Farm	.20	32,776
Residence at 16: Small city (under 50,000)	.31	32,776
Residence at 16: Medium-sized city (50,000-250,000)	.14	32,776
Residence at 16: Suburb near large city	.09	32,776
Residence at 16: Large city (over 250,000)	.15	32,776
Residence at 16: Unknown/missing	.002	32,776
Parent education missing	.07	32,776
Parent high school dropout	.44	32,776
Parent high school graduate	.37	32,776
Parent associates degree	.02	32,776
Parent college graduate	.11	32,776
Teen division/cohort variables		
CL 9 or higher in Census division at age 14	.25	32,776
Pupil-teacher ratio in Census division at age 14	26.9	32,776
Relative teacher salary in Census division at age 14	1.4	32,776
Presidential voter turnout in Census division, ages 13-16	.561	32,776

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